the ray as deduced from measured values of the coefficient of refraction is distinctly higher, being about six and a half times the earth's radius, or about 26,000 miles.

Both letters contain an inadequate presentation of the facts of atmospheric refraction in that they assume that the path of the ray is circular. A very much fuller investigation is necessary in order to account for the distance of the visible horizon or its depression below the true horizontal.

Starting with the assumption that the atmosphere is in static equilibrium, leading to the differential equation $dp=-g\rho dh$, and with the pressure-density-temperature law $p=\mathrm{C}\rho\mathrm{T}$, a further assumption must be made before a complete solution can be arrived at giving pressure, density, and temperature in terms of height. A simple assumption to make is that of a uniform temperature-gradient expressed as $\mathrm{T}/\mathrm{T}_0=1-\alpha h$. For the isothermal conditions $\alpha=0$; for the adiabatic, α corresponds to a drop of 1° C. per 330 ft. increase in height. The integration of the general equation leads, provided h does not exceed 200 or 300 ft., to $p=p_0-g\rho_0 h$ and $\rho/\rho_0=1-(g\rho_0/p_0-\alpha)h$. Since by Dale and Gladstone's law ρ is proportional to $n-1$, we obtain without difficulty

$$n=n_0-0\cdot00029(g\rho_0/p_0-\alpha)h.$$

Further, it is not difficult to show that with a ray that is nearly horizontal the radius of curvature σ is given by the approximate equation

$$1/\sigma=0\cdot00029(g\rho_0/p_0-\alpha).$$

This equation will give any value we like for σ provided we assume a suitable temperature-gradient. If we put $\alpha=0$ (the isothermal state) we get substantially Mr. Mallock's figure. If we take the adiabatic gradient the radius is about 20,000 miles. If we take a fall of temperature of 1° C. per 200 ft., Dr. de Graaff Hunter's value results. A gradient of 1° C. per 100 ft. gives a flat ray and an atmosphere of uniform density. To obtain greater curvatures than Mr. Mallock's figure the temperature-gradient must be reversed.

It is no use to take this formula and expect it to be uniform over even very narrow levels when close to the surface of the sea. The temperature-gradients in the first 30 ft. (the average height of the bridge of a ship above the sea) are very frequently greater than any of the gradients mentioned above, and show wide variations in that space. In such case the path of a ray from a visible object more than a mile away is nothing like circular, but may have variations in its curvature of 300 or 400 per cent. I am aware that the value of the coefficient of refraction mentioned by Dr. de Graaff Hunter is used in books of nautical tables in computing the dip and distance of the sea horizon, but I am aware also that actual measurements of the dip at sea show that tabulated values are frequently in error, sometimes even of the wrong sign! Measurements made by Blish off the coast of California showed that a zero dip is quite possible. In the Red Sea the sea horizon is often refracted above the true horizontal.

Consider the path of the ray of light from the horizon to the observer's eye when the dip is zero. The path touches the earth's surface at the horizon and touches a concentric sphere of perhaps 30 ft. greater radius at a point only six or eight miles away. The radius of curvature of the ray must be greater than the earth's radius at the horizon and smaller at the observer—a maximum at the first point and a minimum at the second. Neither Mr. Mallock's figures nor Dr. de Graaff Hunter's can deal even approximately with a ray-path of this nature, and I think it may be asserted without question that to take adequate account of the path of rays of light through the lower levels of the atmosphere demands consideration, not only of the curvature of the ray-path, but also of the first and second differentials of the curvature. THOS. Y. BAKER.

Admiralty Research Laboratory, Teddington,
Middlesex, December 22.

The Message of Science.

Two great questions are raised in the abridgment given in NATURE of December 22 of the notable address delivered by Sir Richard Gregory during the Edinburgh meeting of the British Association. They are:—(1) How can an interest in, and respect for, science in all its branches, with their essential unity, be developed locally? (2) How can the work of the British Association be so broadened and improved as to ensure that it will yield—to use the words of Sir Richard Gregory—"a statement of ideals and of service, of the strength of knowledge and of responsibility for its use"?

Local scientific societies consist of three types:—(1) Sectional bodies interested in general engineering problems or in the technical details of certain sciences applied to the chief industries of the district. (2) Natural history societies or field clubs. (3) Literary and philosophical societies which provide in a few large towns a good library and series of winter lectures.

With regard to the first type little need be said. They fulfil their specialised functions fairly well, but their work would be greatly improved, and made gradually more attractive, if it were possible to secure an outlook on the broad field of science. Sir Richard Gregory said in the course of his address in Edinburgh: "Whatever Labour may declare officially, it is scarcely too much to say that artisans in general show less active interest in scientific knowledge now than they did fifty years ago." This statement is true, not of artisans only, but of all classes. The demand has been made on science: "Make us rich and comfortable." Science, in a large measure, has responded. But with wealth and comfort has come a lessening of respect for knowledge. The highest things that science can give—an ardour for truth, the power to rise above sordid interests, the desire to become co-workers in an infinite process by which soul is drawn from matter—have been set aside, and we have been landed in a back-wash.

I do not think any revolutionary changes are necessary locally in order to bring back the enthusiasm which linked science a generation ago to human liberty and human justice. Sir Richard Gregory speaks of a federation of local societies "to proclaim the message of knowledge from the housetops." It may be necessary, first of all, for these societies to find out what the real message of knowledge is, but they need not wait for perfect vision; much can be done while they are only groping.

What is wanted, above all things, is such an infusion of earnestness as will arrest the displacing power of the mere lantern lecture. The lantern has been a good servant, but it is threatening to become a bad master. At present the great trouble of the secretary of a literary and philosophical society is to make his organisation pay its way. The chief thing for which the organisation stands is often sacrificed in the attempt to secure popular support. This attitude must be abandoned even if abandonment leads into the wilderness. The message of science will come back from there with renewed constraining power. There are thousands waiting for the message. What

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stands in their way is the lack of faith and of courage on the part of the present directing agencies. Probably the most practical suggestion of immediate value that has been made is that the various scientific societies in any town should arrange meetings for study and discussion—the discussion which seeks agreements and does not emphasise differences—and that the underlying, but not obtrusive, object of the meetings should be the progressive connecting of science with individual and corporate conduct.

The second question has many factors in common with the first. The British Association has suffered from the mental reaction which set in a quarter of a century ago. It is a much smaller factor in the thought and life of the age than it was a generation ago. I think the first helpful change desirable is the recognition of a new principle in the selection of a president and in the making of his annual address. Above all things, the president should stand for the unifying of the sciences, and his address should make some definite contribution to that unity, even when it is built largely on the recent achievements of one section of knowledge.

It is only through a conception, becoming ever clearer, of this unity that science can become the "chief formative factor of modern life." The yearly appeal of a president may do much, but more would be achieved if a day were set apart for the study or discussion of the thoughts and facts he has communicated—a study or discussion, I say again, which should emphasise agreements and not differences.

Progress in this direction might receive a healthy impetus from the universities. They, too, have lost a considerable amount of influence. They are not, at home or abroad, leading humanity. The note of real universality is departing from them. Here again the first practical improvement will come from the manifestation of greater care in the selection of the principals of the attached colleges. They ought to be something more than skilled administrators. They, too, have a great unifying function.

This unifying work might be facilitated if there were periodical meetings of the various professors and lecturers for the study of unifying problems. That does not, I admit, promise to help them immediately to overcome the financial difficulties which are now laming them to a terrible extent and driving them to seek greater support from an overburdened State. But if the universities, encouraged by a steadily increasing enthusiasm for science locally and centrally, were themselves to become again great inspirers of thought, they would soon cease to be troubled by the lack of pence.

W. ROBERTSON.

Middlesbrough, December 29.

Cohesion.

THE theory of cohesion put forward by Dr. Herbert Chatley in NATURE of August 18 is logically based on those of other investigators, and, consequently, does not involve any new element. In all these theories cohesion is made to depend on centrally directed forces which follow either the inverse square law of gravitation or electrical attraction, or that of some other inverse power higher than the second. Dr. Chatley says: "It is difficult to conceive of one force having all these properties, but perfectly simple to imagine an attraction and repulsion combined that will do so, provided that *the attraction decreases more slowly with separation than the repulsion.*" He takes the ground that the force of cohesion as stated by him is related to those following the inverse square law, and that the question of the relation between them is of great importance.

Now it is a matter of common observation that two free liquid spheres on coming into contact with each other always coalesce. The force which causes this is evidently a force *enveloping* their masses, and not a force attracting them. This enveloping property of surface tension was noticed by Maxwell and others; but the theory which makes it depend on molecular attraction renders it impossible to conceive of such a force as enveloping *molecular* masses.

The present writer has adduced (*Phil. Mag.*, June, 1921) very strong, if not conclusive, evidence that the same force which causes liquid spheres to coalesce also causes the free molecules of a gas to coalesce or cohere. It cannot, therefore, be explained by molecular attraction. The alternative is that *it is an elemental force acting, not in lines, but over areas.* As such it is a universal property of the surface of both liquid and solid mass extending to molecular dimensions.

Fortunately, however, there is very definite and easily verifiable evidence that cohesion, and adhesion also, is due to a surface force, whatever its nature may be, as can be seen from the following simple experiments which will be published later in fuller detail.

Spheres of mercury, ranging from 0·05 mm. to 1·5 mm. in diameter, were hung from a drop of water wetting a glass surface above. Each one fitted into an inverted hemispherical cavity in the water, with a well-defined angle in the contact-circle where the water surface joined the mercury surface. With a specially adapted microscope the diameter of the sphere, the width of the contact circle, and the angle between its water arm and the vertical were measured. From these measurements, W, the weight of the mercury sphere, and T, the vertical component of the pull of the water surface on the mercury, were calculated for a large number of spheres. The results showed that for small spheres T greatly exceeded W, but tended to become equal to W when the spheres were at the point of falling off. The ratio T/W decreased gradually from about 6 to 1, thus showing that the surface force of the water pulling on the mercury in the periphery of the contact area was more than sufficient, except in the limit, to support the weight.

Similarly, mercury spheres, with diameters from 0·05 mm. to 2·25 mm., were suspended from a horizontal glass surface. They were attached to the glass either directly or by suspending them from water as before and allowing the water to evaporate. The mercury surface was joined to the glass surface in the periphery of a wide circular contact area, and formed a definite angle with the glass surface. Measurements were made as before, and W and T (for mercury) were calculated. The results showed that, as the spheres increased throughout the range, the ratio of T/W decreased from the surprisingly large number of more than 6000 to about 2. Had T, however, been calculated from $\sigma=270$ instead of $\sigma=547$, the decrease would have been from about 3000 to 1 as before, and hence 270 may be regarded as an approximate value of the surface tension of glass. The increasing values of T/W for the smaller particles would account for the persistence with which molecules of a gas condense on a glass surface.

Further, small particles of any insoluble solid become attached to any surface above by the evaporation of a connecting water drop; or, if the particles be clean and small, they become attached to any clean surface by simple contact with it. This is amply confirmed by extended observations.

Now there is no reason to think that the force of cohesion is not of the same nature in the case of two solids as it is in the cases of a liquid and a solid and

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